that are visible accompaniments of luminaries standing above the horizon. In both classes the light pillar lies in the luminary's vertical; those of the first class are only above the luminary; those of the second class may be either above and below it, or only above it. Bravais has called the first group "light pillars of the first order" and those of the second group "light pillars of the second order." Both classes are to be explained as due to reflections from the basal planes terminating columnar ice prisms unmodified by pyramidal faces, as they float in the air.

Light pillars of the first order are essentially due to the simple reflection of the sun's rays from the lower bases of hexagonal prismatic ice needles, when the sun is below, just in, or very close to the horizon. Naturally such prisms are directed vertically when falling through the air [see, however, the criticisms by Charles S. Hastings on p. 619], but they must also be oscillating slightly in order to produce the appearance of a light pillar. If the falling prisms are not oscillating then such prisms can produce only a reflected image of the sun at rest and such an image would appear to be just as far *above* the horizon as the luminary might be below the same. As soon as the luminary reached the horizon the reflection would disappear. On the other hand, when the elongated vertically directed prisms oscillate, then the amplitude of the oscillations determines how high the sun may stand above the horizon before the resulting light pillar fails to appear. Suppose, to begin with, that the oscillation amounts to 10° and is always in the vertical of the luminary, then when the latter attained an altitude of 10° every reflection to an observer's eye would cease. If the luminary were in the horizon, however, then, since a reflection passes through twice the angle of the mirror, the oscillation would stretch out the image of the luminary to an altitude of 20°, i. e., the image would form a light pillar 20° in height.

Light pillars of greater heights have been observed, and the cause of those 30° and more in height is still a matter of discussion, even for cases where the sun is several degrees below the horizon (of course in the latter case the pillar has the red color of the low-lying sun). I consider it certain that an oscillation of as much as 20° frequently occurs, and that it is not impossible for even greater amplitudes to occur.[2] It is true one may still assume that even the triple reflection would also furnish a sufficient number of luminous rays to contribute to the formation of the upper structure of the pillar, although there must be a considerable difference in the intensity. Thus, suppose a ray reflected from a lower basal plane and on its path to the eye intercepted by an upper basal plane in a favorable position, and that it is reflected upward from this plane to a second lower basal plane also favorably located, then the reflection from this latter surface is the third reflection and can bring the ray to the observer's eye. The writer, however, would resort to the phenomenon of threefold reflections only in the extremest cases of light pillars exceeding 40° in vertical extent. The difference in intensity between simple and threefold reflections, and particularly between threefold and fivefold, etc., is too great to permit observers to over look the strikingly different degrees of brilliancy that must result therefrom if the pendulation remains small. Specially favorable conditions may, indeed, produce a more gradual gradation in intensity; but I here maintain that one is not compelled to follow Bravais who, for the sake of consistency, must hold to the theory that the prism is practically fixedly vertical because his theory of the upper tangent arc of the halo of 46° demands this condition. We may at once assume that the pendulation amounts to 20° or 25°, and if need be may even assume that this value is exceeded, for we find it quite in the nature of things that small floating crystals may be forced quite far from the vertical as they fall through the air. The actual blinding brilliancy of these light pillars argues for a single reflection as their origin.

[2] Bravais endeavors to show that an oscillation of only 4° is sufficient to produce these greater heights if one also calls upon the phenomenon of multiple reflection. He is forced to some such recourse, since he is unwilling to depart very far from his assumption that vertically floating prismatic needles are always free from oscillations. (*See* his Mémoire sur les halos, etc. Paris, 1847, pp. 168-169.)

When the pillar is seen continued beneath the sun as the latter stands somewhat above the horizon, then the pillar is indeed to be referred to a threefold reflection. The width of these pillars is, however, greater than the solar diameter and for the reason that the pendulation of the prisms is not only in the plane of the sun's vertical but in all directions. For this reason the image of the luminary appears to suffer great longitudinal distortion, just as does the reflection of a light on a wavy water surface, and also appears somewhat wider although of course insignificantly so as compared with the lengthening. The light pillars undergo the same slight widening as a result of the prism pendulations not being restricted to the plane of the luminary's vertical.

Light pillars of the second order appear only during higher altitudes of the luminary. They are due to a twofold reflection from the basal planes of the vertical ice prisms, and appear above or below the luminary according to the relative positions of the two reflecting surfaces. Of course the pillars may appear above and below the luminary simultaneously. The light rays fall first upon an upper basal plane, whence they are reflected upward to a lower basal plane which throws them down to the observer.

ON HALOS.

[*Extract from* "Light" by CHARLES S. HASTINGS.]

An incisive and important work on halos, and their phenomena and theory is contained in the latter part of the work entitled "Light"[1] by Prof. Charles S. Hastings, of Yale University. As this work is almost unknown to our meteorological observers, we reprint, by permission of the author, the concluding pages, 221–224 of Prof. Hastings's text.—C. A.

As this completes the explanation of all known features of the complex phenomenon called the halo, it may be well to collect them in tabular form. We will first give those of which the origin has been known for a longer or shorter time, with the name of the physicist who first found the true explanation.

1. Halo of 22° radius. Mariotte.
2. Parhelia of 22°. Mariotte.
3. Oblique arcs of Löwitz. Galle.
4. Tangent arcs to the 22° halo, which become the circumscribing oval with high sun. Young and Venturi.
5. Halo of 46° radius. Cavendish. (Unless objections given on page 219 [of the above-mentioned volume] in regard to this feature are valid.)
6. Horizontal tangent arcs to the 46° halo. Galle; perfected by Bravais.

[1] Hastings, Charles S. Light. A consideration of the more familiar phenomena of optics. New York, Chas. Scribner's Sons, etc. 1901. xi, 224 p. illus. 8°. (Yale bicentennial publications.)

To these must be added the following, which have not hitherto been explained at all, or wrongly explained because grounded upon theories which are untenable:

7. Lateral tangent arcs to the 46° halo.
8. Parhelic circle.
9. Paranthelia.
10. Anthelion.
11. The arcs above and below the 22° halo.
12. The short oblique arcs through the anthelion.
13. Spiral arcs through the anthelion.
14. Vertical columns.

There is, however, a celebrated halo that contains a feature not mentioned in the list, which has given a great deal of trouble to writers on this subject from the time of Huyghens down. It is a rather remarkable halo observed by Hevelius in 1661, and described fully in Smith's Opticks, Volume I, pages 221, 222, although with the exception of this feature it seems to have been a well-developed halo depending upon the presence of the *A* group for its chief characteristics. The exceptional feature is a circle, of which only the lower portions are shown in the figure illustrating it, everywhere 90° from the sun, and therefore a great circle. Bravais, who styles this as the most authentic of all extraordinary halos, cites all the explanations offered, points out their fallacies, but quite frankly declares his inability to propose any more satisfactory theory. Since I am forced to follow Bravais exactly in this respect, it may be well to review the evidence of the existence of the 90° circle, beyond that contained in the original record. There is nothing in the records of the time since Bravais which bears upon this point, at least a search by me has led to no result; hence we are confined to the three examples which that author finds.

The first [example] is found in the description of the halo observed at Melville Island, given by Parry and Sabine. The passage in the last paragraph of the quotation ["Light"], page 143, describing the faint light about a quadrant from the sun, is taken as an observation of the circle in question; but a most casual reading demonstrates that such an interpretation is an entire misapprehension.

The second instance is found in a very uncritical description of a halo seen at Derby in England, in 1802, and published in the Philosophical Magazine, Volume XII, page 373. In this case neither the name of the observer nor the place of the sun in the heavens is given. The passage in which Bravais finds evidence of the 90° circle reads as follows: "* * * the fourth (circle) circumscribed all the others, and was touched upon the western side by part of another of the same diameter." It is quite clear that this circle did not have a radius of 90°, not only because no ordinary observer would dream of calling a great circle of which the sun occupies the position of one pole, a circumscribing circle, but also because in that case another circle tangent to it and of the same diameter would be identical with it. Unquestionably, this fourth circle was the 46° halo, and the circle touching it was the upper tangent arc.

The final case appears to be much more conclusive. It is that of a lunar halo observed by Erman [2] in Siberia in 1828.

Here, with the most minute particularity, that traveler gives the results of his observations, together with the fact that at $10^h 30^m$ p. m., Tobolsk mean time, the measured distance of the moon from the vertex of an auroral arch was 83.2°; moreover, that at the same instant the lunar halo intersected the auroral arch a few degrees to the west of its vertex. This seems very convincing as to the existence of a halo with a radius of 85° to 90°; but reference to the details of the original account shows certain peculiarities which can not fail to awaken strong doubts concerning this conclusion. In the first place, Erman describes the halo without any intimation that it is an unusual one. Then he mentions the fact that it coincided with a part of one of a system of concentric arcs which are supposed to be *auroral* on account of their fixity of position with respect to the earth. Finally, he gives the measured distance of the moon from the apex of the lowermost arch at $6^h 30^m$ in the evening, which he found to be 86°. At this time the moon was close to the horizon; consequently, if the radius of the halo was 90° it would have intersected all the auroral arches nearly orthogonally, and a partial coincidence at any point would have been quite out of the question. But this is not the only inconsistency. An investigation as to the position of the moon at the place given and at the epoch of November 24, 1828, $10^h 30^m$ p. m., shows that its true distance from the point indicated as marking the place of the vertex of the auroral arch was 107°; hence Erman's statement is erroneous.

But it is quite easy to supply to the printed account an emendation which eliminates all the difficulties and contradictions. We find that, on the evening in question, the distinguished traveler was at Sawodinsk, a a place 2° north of Tobolsk, engaged in making a complete and protracted set of observations on the magnetic elements of the place. During the intervals of these observations—important as a part of a very elaborate system—he entered in his notebook the contemporary phenomena of auroral arches and the halo. At the later hour named he made the angular measure, probably with a sextant.

So much is certain. Now let us suppose that he chose the easy task of measuring the distance between the summit of the auroral arch and the nearest point of the halo instead of the less simple task of measuring the interval separating this summit from the relatively brilliant moon, in which case he would have been obliged to experiment with the dark glasses which are not well adapted for this kind of work. Under this supposition and the assumption that the halo was the ordinary one of 22°, we find that the distance separating the apex of the arch and the moon was 105.2°, which accords well enough with the astronomical fact. The only other modification necessary is to assume that the circle which intersected the auroral arch a few degrees to the west of its vertex was the vertical circle through the moon instead of the circle which accompanied the moon. With these highly plausible assumptions the records of a trained observer are made perfectly clear and probable, while without them they are entirely self-contradictory; yet with these modifications the last bit of confirmatory evidence for the 90°-halo of Hevelius falls to the ground. It does not seem unphilosophical to conclude that an inexplicable phenomenon recorded only once in a quarter of a milennium does not really exist.

Addendum of December 15, 1914, by Prof. C. S. Hastings.

In recent letters from Prof. Hastings he says:

"I found Bravais's theory quite untenable, which perhaps had occurred to many others. Bravais's extended collection of records was of great importance to every investigator in this field, but his theories to explain the phenomena were far from happy. The results of my own study in this field are embodied in a book entitled

[2] Erman, Reise um die Erde, vol. 1, p. 544.

"Light" (Chas. Scribner's Sons, New York, 1901), where pages 139 to 153, inclusive, are given to a popular discussion of halos, and in Appendix C, [is given] a more rigid treatment with criticism of Bravais's views together with a substituted theory, which, in my mind, is acceptable. In this I succeeded in explaining all of the authenticated phenomena of halos, 14 in all, with the exception of the famous 90°-circle of Hevelius which would be the 15th. At that time I was almost disposed to question the reality of this feature; now, my attitude is somewhat changed, and I am inclined to a belief that it admits of theoretical explanation. This explanation I have not published because certain of its assumptions are not sufficiently based upon observation. * * *

"The first and most voluminous writer upon the subject, and perhaps the most philosophical, was Bravais. Bravais's fundamental errors were the following: (1) an error in mechanics contained in the assumption that elongated crystals would fall through the air with their axes vertical and plate-form crystals with their axes horizontal; (2) that ordinary reflections from the faces of such crystals could produce anywhere the notable increase in sky luminosity which characterizes the features of halos; (3) that he was justified in assuming the presence of any desired form of ice crystals convenient for his purposes provided that they did not contradict the laws of crystalography, overlooking, moreover, the fact that in order to attain his explanation he must assume the great predominence of that particular type in just the desired direction.

"Writers who have followed Bravais have, to the best of my knowledge, corrected only the first of these fundamental errors, namely, the mechanical ones. The optical and the crystalographical have not been touched, see figures 12, 13, 14, of page 434 of the MONTHLY WEATHER REVIEW for July, 1914.

"Let us consider briefly the significance of the three criticisms above, or rather, since everyone agrees as regards the first, let us turn our attention to the others. When we regard an ordinary feature of the halos, the 22°-ring for example, the origin of which is explained to the satisfaction of everyone, it will be observed that very nearly all of the light which enters a face of a suitably oriented crystal emerges in the direction of the observer other crystals present merely diluting the phenomenon. Now imagine the amount of light sent from such oriented prisms reduced to one-twentieth or less, can anyone suppose that under such circumstances any very marked or even notable increase of luminosity could be found in this region? But this is just the ratio of the decrease of luminosity when one depends, as do all of these writers, upon ordinary reflections from the crystals.

"As to the third criticism, it hardly needs more than a statement to render it valid. That Bravais should have premised a large number of unknown crystal forms merely because he thought they would meet his theoretical requirements is not so surprising; but that anyone else should invent a host of new forms which have an even less probable actuality is certainly very surprising. The tremendous outstanding objection to this method, which appears above, has never been touched upon as far as I know—I mean, that even granting the existence and efficiency of those highly complicated crystals one must put them in enormous numerical majority in just the required direction in order to be effective.

"The principles at the base of my theory are also three, and of the simplest kind: (1) Only such forms of ice crystals as have been observed and are of very simple type can be presupposed. This is an almost self-evident condition since the phenomena necessarily infer an exceptional homogeniety of forms, for otherwise since prescribed forms only are effective, the presence of all others would only add to the whiteness and opacity of the sky; (2) the orientation of the crystals in falling must obey the law of mechanics; (3) all of those features of halos which are attributable to reflections must find their explanation in every case in total reflections.

With these narrow restrictions, made by no other writer, I had [in 1900] succeeded in explaining all well authenticated phenomena of this class with the postulate of only 2 forms of perfectly well-known crystals."

SYSTEMATIC EXPLORATIONS OF THE UPPER AIR WITH ESTIMATES OF COST.[1]

By MARK W. HARRINGTON, Chief, U. S. Weather Bureau.

[Read before the International Conference on Aerial Navigation, Chicago, Ill., August, 1893.]

[This paper is of such historical value that we reprint it in full from the original for the information of students of meteorology. The paper was briefly referred to in the Monthly Weather Review, June, 1897, **25:** 313, and January, 1914, **42:** 39.

Many will be interested to learn that Prof. M. W. Harrington is still quietly living near Philadelphia, Pa.—C. A.]

The exploration of the upper air is the immediate requirement for the satisfactory advance of meteorology. There is abundant reason to think that many of the changes which go under the name of weather have their origin at some distance above the earth; and of what occurs in the cloud layer or layers, our knowledge is insignificant or theoretical. The only systematic attempt to investigate the higher atmosphere has been by means of mountain stations; but this, though it has led to a series of interesting results, does not meet the requirements of the meteorologist. The station on the mountain top is after all only a station on the earth's surface; and though many of the equidynamical surfaces show change with the elevation of the land (the isobaric, for instance), others (as the isothermic and those for wind and humidity) show marked adaptation to the contour of the surface. Many aeronauts have noted this adaptation as especially true of the cloud layers, the lower one often reproducing with some exactness the general variation of the surface below. We can hardly expect, therefore, that the mountain stations, useful as they are, will give us the aid needed in ascertaining what goes on at considerable elevations in the free air.

There are several ways of exploring the upper air by investigating the ray of light which has passed through it. The spectroscope promises much in this direction. The twinkling of the stars might be expected to give us a great deal of information when properly interpreted; Señor Ventosa has shown that even the fluctuations on the margins of the larger celestial bodies, when viewed in the telescope, have apparent relations with the upper winds. This information must, however, be vague, because the total result received by us is the integration of the individual effects at each point of the path, and it is not practicable to separate the sum into its parts. Besides, even if this could be done, the information to be obtained would be very incomplete, as it would relate only to a part of the series of meteorological elements. It may be mentioned as of interest in this connection that the scintillation of stars has been especially and systemati-

[1] *Reprinted from* pp. 349–354 of Proceedings, International conference on aerial navigation, Chicago, Aug. 1, 2, 3, and 4, 1893. Amer. eng. and railr'd jour. New York, 1894, iv, 429 p. 8°.